May 11, 1953

Dr. F. Kauffmann
Statens Seruminstitut
Copenhagen, Denmark

Dear Dr. Kauffmann:

Dr. Edwards has indicated that you might like to have the phage PLT22 which is used in our transduction experiments. I am happy to furnish this material, but as a matter of momentary convenience to myself, I hope you will not object if it is furnished somewhat indirectly.

I am sending a bacterial culture, SW-944. This is derived directly from your S. paratyphi B (b:—) / O 248 in two steps: first, a xxxxxxxxx reversion restoring motility (and the b antigen) was selected for (with considerable difficulty), and second it has been made lysogenic for the PLT22. You should have no difficulty producing large amounts of the phage by simply growing SW-944 together with O 248. In addition, SW-944 works very nicely with Lwoff's method of inducing lysis, and the output of phage, by means of treatment with ultraviolet light.

You will recall having sent me a culture of S. paratyphi A #339, which carries XII_2. I have had only moderate success in experiments with this as a recipient in transduction experiments. However, a b phase has appeared in one experiment, SW-944 —x #339. I am sending this as SW-1047. Would you be kind enough to verify the somatic antigen of SW-1047, which should be the same as that of #339?

The purpose of these tests had been to see whether the XII_2 does play a decisive role as the receptor for PLT22. #339 serves so poorly as a recipient in transduction that the result is ambiguous. Stocker has found, however, that an O form received from Felix, and derived from A17689 S. paratyphi A, could be restored to motility (and the a antigen) by transduction from other motile Salmonella. Perhaps you have had some detailed experience with this strain. At any rate, I am sending it as SW-948. From it, I have secured a subline which is considerably more efficient as a recipient in transduction, SW-1048. It would be important to the question to determine whether XII_2 is detectable, especially in the latter. These cultures, both SW-948 and SW-1048, form rather rough colonies on agar, and this may be related to their inefficiency. If you should happen to have a smoother subculture of the same line, I would be obliged for it.

If you are contemplating transduction experiments, I would recommend your O 248 strain as an indicator. This is so stable that spontaneous motile reversions can be ignored, and functions very well as in experiments in which phage is added to O 248 and the mixture inoculated on plates or in tubes of semisolid agar. Most of the resulting swarms will be b:-; it is an unusual attribute of this particular O form that some of the swarms will have anantigen characteristic of the donor strain, when this is different.

I must ask your apologies for my confusing remarks about the behavior of this strain. About a year ago, we thought that i phases appeared from it not only in the presence of phage grown on S. typhimurium, but also without this.

The single exceptional result has not been repeatable. In every subsequent experiment (involving many hundreds of swarms), i phases have appeared only in the presence of phage from S. typhimurium. The technical error evidently had to do with the incomplete inactivation of phage used in one control experiment; it is possible that the transducing activity of a phage is destroyed by heat less rapidly than the infectivity. The same has been shown for inactivation by ultra-violet light.

P.R. Edwards and I have recently completed a series of general experiments on serotypic recombination, and are preparing results for publicatipn. The types that have been generated include the following:

in group B (IV XII or IV V XII): monophasics: a, b, [illegible] d, eh, gm, gp, i, lz_{28}, r, enx, z_6. In group D, monophasics, a, b, c, d, eh, [illegible]gp, i, r, 1,2,

The diphasics include: B:, i:enx, b:1,2, gp:1,2, d:enx a:enx b:enx a:1,2 d:1,2

and D:, a:1,5 b:1,5 c:1,5 d:1,5 eh:1,5 i:1,5 lz_{28}:1,5 [illegible]:1,5 a:1,2 and a:enx

lz_{28}:enx i:enx

May I mention also a few very peculiar monsters, genetically and serologically most anomalous:

IV V XII i:b , IV V XII a:b, IV V XII 1,2:enx and IX XII 1,2:1,5.

Many of these combinations have, of course, already been listed as serotypes in the Kauffmann-White schema, others have not. If any of them would have any special interest for you, please do not hesitate to indicate your wishes.

I believe Dr. Edwards will already have communicated the experiment with your "gallinarum" strain 151-52, showing a latent gm antigen. I have since had an opportunity to examine several other typical S. gallinarum. These differ from 151-52, inter alia, in their inability to transduce motility to [illegible] 0 248, which is why they had not previously been successfully handled. A IX XII a:—, derived from S. miami —x S. typhi H-901 has proved to function as a good indicator, and in several cases typical S. gallinarum —x IX XII a:— has given g... phases. These have not yet been fully typed. On the other hand, S. pullorum —x IX XII a:— and other indicators have given no meaningful results.

Dr. Edwards had received a culture (his 3821-51) from Floyd [Cairo] via Barnes [Bethesda]. This resembles your 151-52 very closelybindeed. Are these two cultures possibly identical?

Yours sincerely,

Joshua Lederberg